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The question as to the probable cause of this difference is under investigation; and for the purpose of better treatment, a number of occultations of stars, observed at various observatories, are undergoing reduction, so as to obtain a greater number of phenomena to elucidate the question.

On the Rejection of Discordant Observations. By E. J. Stone, Esq., Her Majesty's Astronomer at the Cape of Good Hope.

In the *Monthly Notices* for April 1873, there appears a paper upon this subject by Mr. J. W. L. Glaisher. Mr. Glaisher states that the original object of his paper was to examine a criterion for the rejection of discordant observations, which I had published in the *Monthly Notices* for April 1868. I have read Mr. Glaisher's paper with care, and venture to offer the following remarks on the subject.

The objections to my criterion appear chiefly to resolve themselves into a preference for a general method of treating observations, which Mr. Glaisher has developed at considerable length. This method is regarded as a complete method of treatment of observations by the theory of errors, and is considered to render the rejection of anomalous results unnecessary. But as Mr. Glaisher supposes that before his treatment is applied 'obvious mistakes' are rejected, and my criterion refers only to the rejection of mistakes, I can hardly understand in what manner the proposed treatment is to render unnecessary my, or some other, criterion for the rejection of mistakes. There are no distinct marks upon the mistakes which appear amongst the records of observations, by which they can be infallibly recognized and rejected.

With respect to the statement made by Mr. Glaisher 'that the disqualification of an observation must depend solely upon the magnitude of its supposed deviation from the truth,' I must confess that this statement appears to me obviously incorrect. The deviations from the truth which may be found amongst observations will depend upon the accuracy of the observations. Deviations will frequently appear in one class of observations made with one class of instruments, which would in the same class of observations made with superior instruments be, I presume, amongst those 'obvious mistakes' which Mr. Glaisher would at once reject. When mistakes appear amongst the records of observations, they can only, I believe, be recognized by a consideration of their deviation from the supposed truth *in comparison with the average discordances of the class of observations under consideration*, and then they can only be recognized with more or less probability and not with absolute certainty.

I believe that most observers and experimentalists will admit

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that the number of mistakes which will be found amongst the records of observations will depend upon the carelessness of the observer, the complexity of the observations, the clearness with which the scales, or micrometer-heads, to be read are divided, and the circumstances of illumination of these scales under which the observations have to be made. I believe also that it will be admitted by such persons, that a discordant result, which appeared amongst the records of observations, was more likely to have arisen from a mistake, if the observations were made under unfavourable circumstances, than if made under more favourable circumstances. The only assumptions upon which my criterion for the rejection of anomalous results is based are contained in the above statements.

I assume that a particular person, with definite instrumental means and under given circumstances, is likely to make, on an average, one mistake in the making and registering n observations of a given class. The probability, therefore, that any record of his of this class of observations as a mistake is $\frac{1}{n}$. From the average discordances amongst the registered observations of this class we can find the probable error of an observation in the usual way, and also the probability of an error greater than a given quantity, as C. Then my principle simply amounts to this,—that if the probability in favour of a discordance as large as C is less than that of a mistake, or $\frac{1}{n}$, I am not only justified in rejecting, but, as a reasoning animal, required to reject the discordant observation. I do not pretend that this method affords a principle by which mistakes can be recognized with absolute certainty. It is quite possible, in accordance with my views, that mistakes should occur, too small to be rejected by my criterion. It is quite possible that the result rejected as a mistake may arise from an unusual aggregation of the results of several independent sources of error, each of which has acted upon the other observations. But what I do maintain is (1) that the rejection of a mistake is of far more importance than the omission of an observation from a group. One mistake may destroy the evidence afforded by a considerable group of observations. (2) That the rejection of results which appear amongst the records of observations can only be justified by a consideration of opposite probabilities. (3) That when the evidence in favour of a result arising from the action of the ordinary sources of error is less than that, it arises from sources of error different from those in constant action, and with those results, if I could separate them, I have no concern; then I am not only justified but required to reject such discordant result.

I have never seen any clear objections raised to the principle upon which my criterion is based. I can, however, quite understand that anyone may have serious doubts, before trial, of its practical value. It is no doubt true that the value of n for a

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given observer and class of observations can rarely, if ever, be determined with perfect accuracy. If, therefore, an accurate value of n be required in practice, the method would, as a practical guide, fail. But such is not the case. I consider that one of the most important results of my paper was to show that no assumptions could be made respecting the number of observations which, on the average, were made with one mistake, without leading to a limit beyond which there was a greater probability of an error arising from a mistake than from the average run of errors, and that with any suppositions that could be admitted in practice, the limits of rejection differed but slightly. If the probable error of a result is equal to $0''\cdot477$, and we assume an observer to commit one mistake in the making and recording 100 observations, I have shown that the limit of rejection would be $1''\cdot9$; but if we assume that the observer only commits one mistake in the making and recording of 1,000 observations, the limit of rejection would only be shifted to $2''\cdot3$. Nor is this all. If my criterion be accepted, it follows that for a class of observations in which the liability to mistakes remains sensibly constant, whilst the probable errors of the results vary; as for instance, Polar Distances at different Zenith Distances, then the limit of rejection varies directly as the probable error. Thus assuming one mistake in the making and recording of 500 observations, we should have no more grounds for the rejection of a discordance of $17''\cdot6$ for a Zenith Distance a little greater than 87° than for the rejection of a discordance of $2''\cdot2$ at a Zenith Distance about 26°.

As Mr. Glaisher thinks that "It is pretty clear that it would be a good deal easier to choose a limit of retention at once than to guess a value of n and deduce one," I would ask him to consider the above case and the following. Is it easier to decide for Zenith Distances about 87° with probable eror $3''\cdot82$, whether the limit of rejection should be $7''\cdot6$ or $17''\cdot6$, or to decide whether the observer is likely to be so grossly careless as to make one mistake in every five observations, or so careful as to only make one in five hundred? I have very little doubt in which way this question would be decided by most minds. My criterion, if its principle be accepted, shifts the decision of the first question upon the second. Our mathematics, after all, can do no more for us than to bring the decision of complex questions to more simple forms. This, I believe, my criterion effects to a very considerable extent.

There is one point connected with the applications of this criterion to which I should wish to call attention. It is quite conceivable that in certain cases the number of rejections by an application of the criterion would be greater than could be admitted on the supposition of the discordances arising from mere mistakes. Such a case would have arisen in the discussion of Star places before the discovery of aberration, and in all similar cases. Here, if the number of rejections pointed out by

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the criterion is greater than can be admitted on the supposition of mere mistakes, we are put, with greater or less certainty, according to the number of cases of rejection pointed out above the number of admissible mistakes, upon the trace of sources of error which act with variable intensity upon the different results or upon the discovery of new laws of nature.

Before offering any remarks upon the method of treatment of observations which Mr. Glaisher advocates, I must call attention to some miscellaneous remarks scattered over his paper, which appear to me to indicate, that, although Mr. Glaisher accepts the results of the ordinary theory, he does not accept the principles upon which alone, so far as I am aware, these results can be proved. For instance, I entirely demur to the principle of Mr. Glaisher's remark—"Now, errors may arise in a perfectly legitimate way (that is to say, by the accumulation of smaller errors in the way contemplated by the theory and independently of the observer's care), and yet their retention may be quite as disastrous as if they had been mistakes made by the observer." The principle of this statement is, in my opinion, entirely opposed to the first principles upon which the law of facility generally adopted is based. I presume, however, that this statement must be connected with another—"The continuous errors are bound to look after themselves if the number of observations is infinite, but not otherwise, and in that case there is no distinction between continuous and intermittent errors at all."

This is the mere language of the schools, to which an answer, equally satisfactory, might be given in the same language. Although it is true that the continuous errors will not look after themselves until the number of observations is infinite, yet an infinity of a different order is required before the intermittent errors will be found to do so. Practically, however, we have nothing to do with infinities of observations.

It is only so far as we can legitimately assume that the mutual destruction of errors which would take place in an infinity of observations will take place sensibly in the number of observations with which we have to deal in practice, that we can transfer the theorems of the calculus of probabilities to the discussion of the results of observation.

In such cases it is most certainly not true, because we may assume as a close approximation to truth that this mutual destruction of errors takes place for the continuous errors, we may with equal truth assume that it takes place for intermittent errors arising from causes which will act only once in two or three hundred observations, and which, when they do act, may produce errors enormously larger than those arising from the causes in continuous action. These remarks meet, I believe, all Mr. Glaisher's statements respecting my criterion. (*Monthly Notices*, vol. xxxiii. pp. 398 to 400.) We have not, in practice, to deal with such numbers of observations that the average

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number of observations with one mistake is a very small fraction compared with the number of observations discussed.

Unless this is the case, Mr. Glaisher's remarks on page 400 have not, in my opinion, the slightest bearing upon the practical aspects of the question. In my paper of 1868 I pointed out what I believe is a valid objection to a criterion proposed by Professor Chauvenet, viz.:—that whenever the probability of an error in n observations was less than $\frac{1}{2}$, the corresponding observation should be rejected.

My objection was thus stated:—"In $2n$ such observations we ought reasonably to expect an error greater than or equal to x. In n observations, therefore, we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no ground for the assumption of some disturbing cause of error." To this Mr. Glaisher appends the following remark:—"Taking the words literally, it is enough to reply that the question is not how far we are surprised at such an error having been made, but whether it should be retained or not." I do not know whether Mr. Glaisher wishes it to be understood that he thinks the objection I have made unsound; but I presume he would hardly have extracted my remarks upon this point and appended his own unless he did think so. If such be the case, I fear that nothing I can say will make the point clearer to him. My objection simply amounts to this—that if the odds are fairly two to one against an event coming off, and yet the event does come off, we have no right to assume from this alone that unfair practices have been resorted to. There is a statement made by Mr. Glaisher (*Monthly Notices*, vol. xxxiii. p. 402), to which I will now call attention:—"Another point deserving notice is the statement, usually received as self-evident, that positive and negative errors are equally probable. In certain conceivable cases this seems, at first sight, not to be the case; for, suppose one were to estimate the area of a field containing, say, two acres, then it would be quite possible to say four acres, or even six, but absurd to say zero or minus two (a case of this kind was pointed out to me by a pupil in my lectures this term, as an exception to the rule). It is a sufficient justification of such an apparent exception to observe that an estimation (or guess) is not of the nature of an observation, as contemplated in the theory of errors; but still the analogy is close enough to make the discrepancy worth remark." Now, I must confess that I have never heard any one before maintain that positive and negative errors are always equally probable. There are such things as systematic errors recognized in practice, *i.e.* errors which, for an observation made with a particular instrument in a particular way, will always enter into the result with the same sign. I believe that it is usually assumed that it is only for those classes of errors for which positive and negative results are equally probable that theorems which depend upon the ordinary law of facility of errors can be appealed to as means of the elimination of error in the result.

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But this can hardly be Mr. Glaisher's meaning, for such a view would have prevented the difficulty mentioned from being raised.

Assuming the law of facility

$$\frac{h}{\sqrt{\pi}} \cdot \varepsilon^{-h^2 x^2} dx$$

for an error between x and $x + dx$

and denoting the n independent results by $a_1, a_2 \ldots a_n$; $a_1 - x$, $a_2 - x$, &c., by $x_1, x_2 \ldots x_n$, and the corresponding measures of precision by $h_1, h_2 \ldots h_n$, we have for the probability u of the coexistence of the errors $x_1, x_2 \ldots x_n$

$$u = \pi^{-\frac{n}{2}} \varepsilon^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + \ldots)} h_1 . h_2 \ldots h_n (dx)^n$$

Then assuming $h_1 = h_2 \ldots = h_n = h$ a constant, Mr. Glaisher deduces, in accordance with the ordinary theory,

$$x = \frac{a_1 + a_2 \ldots + a_n}{n}$$

With this result, the probabilities in favour of $x_1, x_2 \ldots x_n$ are computed.

Mr. Glaisher then proceeds to obtain a result which he considers more probable than the arithmetical mean, as follows. Having found the probabilities (on the assumption made, viz. :—that $h_1 = h_2 \ldots = h_n$ in favour of errors $x_1, x_2 \ldots x_n$ to be $p_1, p_2 \ldots p_n$ he weights the results in accordance with the probabilities, and determines x from the condition that it shall make a maximum of

$$\varepsilon^{-[p_1^2 (a_1 - x)^2 + p_2^2 (a_2 - x)^2 + \ldots]}$$

This value of x is considered as more probable than the arithmetical mean, and Mr. Glaisher recommends that the process should be continued until the consecutive values of x come out sensibly the same.

I admit that whenever a large discordance from the arithmetical mean is an indication of the action of sources of error of an abnormal character, less weight should be given to the corresponding observation; but instead of regarding Mr. Glaisher's method as mathematically complete, I regard it as mathematically unsound, even upon the assumptions made.

If we are to ignore all *a priori* considerations, and to adopt as the most probable result the value of x which makes

$$u = \pi^{-\frac{n}{2}} \varepsilon^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + \ldots)} h_1 . h_2 \ldots h_n (dx)^n$$

a maximum, where $h_1, h_2 \ldots h_n$ are to be considered, as Mr. Glaisher supposes, unknown quantities whose values are to be assigned from a consideration of the mere run of the numbers in the question before us. Then clearly $h_1, h_2 \ldots h_n$ are